SCIENCE

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THE OUTLOOK FOR SCIENCE[1]

THE most remote origins of science are to be sought in the early observations of primitive races of men. At first phenomena were probably registered in memory with no attempt to relate them other than by means of the hypothesis that they were due to the will of some intelligence akin to that of man. It appears that an enormous period of time elapsed before men began to conceive even the possibility that these phenomena were bound together by laws through which they were capable of explanation. A long preparation of experience seems to have been necessary before this conception could arise.

But we are not to look back upon this period as barren. It gave rise to one thing at least of essential importance, namely, the effort to relate phenomena in such a way as to make the universe intelligible. It matters little what particular explanation was first offered; but it was a thing of profound importance to have conceived the possibility of any explanation at all.

The preliminary forms of this conception have probably been lost from the view of history. The first name which appears on the record as we now have it and indeed the first name in the history of European thought is that of the Greek philosopher Thales. He sought to go behind the great multiplicity of phenomena in the hope of finding a deep unity from which all difference had been evolved and by means of which these phenomena might themselves be explained.

It is interesting to note particularly that in this first attempt to make the universe intelligible Thales sought to ground everything in a single material cause. This he found in water. How he related it to the plurality of phenomena is not known. It is certain, how-

[1] An address delivered to the Indiana Chapter of the Society of Sigma Xi on November 5, 1914.

ever, that he set his contemporaries to thinking along a new line. Other explanations were offered each of which sought to find a basis for all phenomena in some one material substance. One of these was air. Another was a hypothetical substance having properties between those of air and fire. We need not mention more of these. It is sufficient to observe that it was hard to offer a reason why one of them afforded the desired explanation rather than another. One outcome, however, of this discussion among these thinkers is very interesting, namely, the conclusion reached by Anaxagoras that all things have existed in a sort of way from the beginning, but that originally they were in infinitesimally small fragments of themselves, endless in number and inextricably combined throughout the universe but devoid of arrangement. These fragments were the seeds of all things. The gradual adjustments of these among themselves have given rise to all phenomena whatsoever.

Thus ended the first search among the Greeks for a single material cause of all things. There followed a long period in which science no longer proposed to itself such an ambitious problem. In modern times each worker has been content to consider a narrow range of phenomena and seek a particular explanation. For a long time we have proceeded in this way with the study of special problems. In recent years we have been brought back in a most surprising manner to face the old problem of the Greeks. In the meantime our chemists and physicists had studied all known substances and had found that they were composed of about seventy elements.

When we had become thoroughly convinced that these elements were separate and distinct, radioactive substances made their appearance in our laboratories and we were compelled to revise our old opinions. Emanations of various sorts were then eagerly examined and before long it was realized that various of these seventy elements were giving off the same sort of electrons, so that they must certainly have something in common. Moreover, some elements were actually transformed into others.

In view of these facts one could hardly fail to raise the inquiry as to whether all elements are not indeed only different combinations of electrons. The speculative hypotheses of the old Greeks in the earliest period of scientific history thus stand prominently before our physicists in their laboratories to-day. The striking elements of agreement between a theory asserting that all matter is made up of electrons and that of Anaxagoras with its primal fragments of things are very remarkable, to say the least. What is done with this old problem in its new form will certainly exert a marked influence on scientific progress.

Looking from a certain point of view one may say that the great problem of science is to find out just what unities do exist among phenomena. If we can not trace everything to one cause we shall at least seek to find those general laws by means of which the greatest number of phenomena may be explained. This we must do in self-defense; otherwise we should soon be helpless before the enormous volume of science. Only if we grasp the great fundamentals, which include many particulars, shall we be able to continue our progress.

Economy of energy is one of the great demands which will press itself upon our attention with increasing force as the body of science is enlarged. One way to realize this economy is to make permanent conquests which remain for all time our possession. This is done in the science of mathematics. Other sciences should strive for the same permanence, but be all the time ready to grant that it has not been attained. No law of phenomena should ever be counted so well established as not to be subjected to every further test which ingenuity can devise. Over and over again our fundamental steps of progress have been taken in the most surprising way in fields of thought where everything had apparently been examined with the greatest care.

The way in which the mathematician has gained economy of energy through permanence of result is instructive. He confines his attention within limits so restricted that he may define his terms and ideas with the sharpest precision. In doing this it may be necessary to leave out of account a considerable part of

the problem in which he is interested. But the results which he obtains are permanent; these in turn he may use to arrive at tentative conclusions concerning the other parts of his problem.

In like manner it may be necessary that a theory in experimental science should restrict itself to a certain point of view in order to remain scientific. The range of phenomena, even in a restricted field, may be too great to be taken account of at once. Therefore some elements are left entirely out of mind until considerable progress has been made with the investigation. This was done in the case of the kinetic theory of gases, the size of the molecules being taken into consideration only after extensive investigations had been made in which this element was ignored.

Such a plan of procedure will cause us no uneasiness if we remember the guiding purpose of physical science. It does not attempt to afford us an explanation of the essence of things; if it did so it would find itself amidst inexplicable difficulty from the beginning. Its purpose, on the other hand, is to give direction to our researches into details and to afford us the best means of acting on things and of predicting phenomena.

It may very well happen that a "false" theory will serve this purpose better than a "true" one. In other words, a theory which is in agreement with only a narrow range of facts may be better for us at a given time than one which agrees with a much wider range. The one more nearly perfect, in the absolute sense, may be out of reach of our proper understanding or at least beyond any means of experimental verification at our command.

As a first example of this let us consider the case of a savage who has been accustomed to take the animistic view of nature. It may very well be true that his primitive theory brings helpful ideas and enables him to get around in his world and interpret it in a satisfactory way. His observations have little of precision about them and consequently they do not clash with his theory. To this creature the Newtonian law of gravitation would be meaningless and useless. For him it could serve none of the ends for which we employ that or any other scientific theory. For him to make such a hypothesis as this would be distinctly unscientific.

Another case in point is the old Greek theory of which we spoke a few moments ago. According to it all matter had a unique origin, and a primary task of the philosopher was to discover what substance gives rise to all others by the combination of its parts. None of the answers which they were able to arrive at, as we have seen, were of such character as to give them greater power to act on things or to predict phenomena. In accordance with a true scientific instinct the theory was therefore allowed to drop out of mind. Nowadays it has been revived in a different form because in this form it now seems capable of being subjected to experimental examination.

Probably the best example of the difficulties of a position where speculation has outrun observation is afforded by the atomic theory of the ancients, a theory which is very close in its general aspects to that which is usually accepted at the present day. In recent times this theory has given rise to the most important and far-reaching investigations. It has in a remarkable degree all the characteristics of a useful theory, which we enumerated above, and in many ways has proved itself vital in experimental investigations. Among the ancients, however, it seems to have led to nothing but speculations and disputings. It was too far in advance of other parts of scientific theory to be amenable to experimental investigation. Though essentially in agreement with facts, as we understand the matter to-day, it yet led to no scientific conquests in ancient times.

Such examples as these remind us that we should not set ourselves the task of finding the "true" explanation of things. From a scientific point of view our plans should be far less ambitious. This is a point, it seems to me, which we should be careful not to lose sight of. What we want to do is to frame general laws which to us appear to be the simplest we can find and which have the following three properties: they are in accord-

ance with all known phenomena; they enable us to predict events; they suggest to us new experimental investigations to carry out. We shall not undertake to say that these laws are true in any absolute sense. Furthermore, it will not cause us any uneasiness if we find a new phenomenon which contradicts one or more of them. That is a thing to be expected if we are making progress. It will be no surprise if a principle which was developed to relate past experiences should turn out to be insufficient to deal with future experiences.

The experimentalist is thus continually finding things which run counter to his preconceived opinions, whether they are based on unreasoned intuition or on large collections of facts. It is important to us to analyze the way in which men have heretofore met such situations. They will continually arise in our experience as long as we are making progress. From the most superficial examination we may see that they have often stood in the way of advancement.

When an opinion has gained a strong hold on our imagination it may obstinately refuse to be removed although it causes us grave trouble to keep it in agreement with facts or even leads us into contradictions from which we can find no escape. The early history of astronomy furnishes us with a good illustration of this matter. The Pythagoreans undertook to make precise the central problem of this science. Plato followed with other work along the same line. By means of a considerable range of speculation and reasoning, which would have little weight with us today and therefore need not be repeated now, these philosophers came to the conclusion that uniform motion in a circle is the most perfect of all motions, and therefore must be that of celestial bodies. But it was obvious that a simple motion of this kind for each of these bodies was insufficient to explain their positions at various times. Thus from the outset it was apparent that it would be necessary to consider the compounding of various circular motions in order to account for observed facts. Therefore those early thinkers confidently proposed as the fundamental inquiry of theoretical astronomy the following questions: How can we explain astronomical movements by means of uniform circular motions?

It was well to have this problem proposed, although it turned out to be incapable of solution. Directly or indirectly it has exerted a profound influence on the progress of every science. As long as the body of observation was sufficiently meager men could labor with some hope of answering the question as proposed. At first it was sufficient to compound two or three motions. After observations became more exact it was necessary to put together four or five circular motions for one body and to introduce numerous hypothetical spheres in order to have something to move along the requisite circular arcs. This thing continued till the explanations bewildered one with their complexity. Still men held to their preconceived idea of circular motion for many centuries until Kepler finally broke the spell by the discovery of the three laws on which modern theoretical astronomy is based. It is instructive to all scientific workers, I believe, to ponder the experience of men in dealing with this old problem.

As another example of the influence of preconceived opinion consider the old belief of chemists that the formation of organic compounds was conditioned by a so-called vital force. In accordance with this theory it should be impossible to synthesize organic compounds from dead matter. But in 1828 Wöhler succeeded with the synthesis of urea. But the belief in the necessity of a vital force died hard. Men tried to get around the new fact by supposing that urea stands midway between organic and inorganic substances. But the accumulation of other cases in which organic compounds had been synthesized finally led to the rejection of vital force as a factor in purely chemical relations.

A very curious case which was obviously in disagreement with facts is afforded by the old phlogiston theory of combustion. According to this theory combustibility is due to a principle called phlogiston, which is present in all combustible bodies in an amount proportional

to their degree of combustibility. The operation of burning was simply equivalent to the liberation of the phlogiston. This theory dominated chemical thought for more than a generation, notwithstanding its inherent defect due to the fact that the products of combustion were heavier than the original substance, whereas the theory demanded that they should be lighter.

I have purposely illustrated the influence of preconceived opinion by means of some of the older examples. Many others might be given. In fact, in nearly all our theories relative to experimental phenomena we introduce important elements not suggested by our observations, but by our own esthetic sense. Witness the introduction of the ether in so much of physical theory. A man sometimes feels that he is putting into his theory nothing except what observation has directed. This, I believe, is always a delusion. Moreover, I think that it is an undesirable thing to attempt. It is not true that observations compel any one theory. In fact, as Poincaré has shown, there is an infinite number of explanations of any finite set of facts. From among this enormous totality we must select the explanation which is most satisfying for us from considerations of convenience or from the demands of the esthetic sense. This is actually what we always do. It should be done consciously.

Now it is clear that any body of doctrine built up in this way is in danger of being seriously in error, and therefore it is necessary for us often to reexamine our theories with a view to ascertaining whether the preconceptions which were wrought into them still appear to be justifiable. This is one of the hardest tasks of the scientist. Accordingly he often waits long in the presence of his difficulties before he tries to overcome them by this heroic method. He is usually more averse to the surgical knife operating among his ideas than on the members of his body, however hard he may try to overcome this disposition.

It is no surprise that this is so. The race was too long practical before it sought to become scientific for us to make the change readily. Some one has defined the practical man as one who practises the errors of his forefathers. He is tied down to his preconceived opinions, not being enough of a dreamer to get away from them. He will be able to get through the world without receiving many hard knocks; but he will not inaugurate profound changes and advances in human life. That will always be left for the scientist who refuses to be satisfied with what is and who is always seeking a new sort of fact to destroy his own and his contemporaries' equilibrium.

But this will be harder for him to do as the years pass. In fact it is true in one respect that the problems of the scientist are increasing in difficulty. As the mass of accumulated observations grows larger there are fewer essentially new facts to be discovered. And when it becomes necessary to devise a new theory it is harder to make it fit into and explain the great array of recorded phenomena. But this affords no ground for pessimism, as we shall show in a moment. Moreover, it carries with it a reward of its own. If a theory can be made to fit into the facts as now known it has a good chance of doing service for some time, and this from the reason that it has been made to explain so many things already.

But there was a real advantage to be gained from the meagerness of data in the old time. It was not so difficult to theorize with some appearance of success, and therefore men the more readily conceived the possibility of relating things according to law and the more easily set up a tentative explanation. I have no doubt that speculative philosophy, for instance, has profited in times past by the meagerness of the data on which its speculations were based. The very fact that no large body of observed occurrences stood in the way of speculation emboldened men to launch forth upon what otherwise would have been a forbidding sea.

But confidence in setting forth did not save from danger and shipwreck. For some time we have known that no conclusion in science

is safe unless it is built up from a large collection of facts. Our philosophers are beginning to realize that the same sort of thing is true in their realm, and hence we should not be surprised to see science itself conquer a large part of the ancient domain of philosophy. Progress in this direction has already been sufficient for men to begin to speak definitely of "the scientific method in philosophy." Such indeed is the title of the volume containing the Lowell Lectures delivered by Bertrand Russell in Boston last spring. The adherents of this new method believe that it represents in philosophy "the same kind of advance as was introduced into physics by Galileo: the substitution of piecemeal, detailed and verifiable results for large untested generalities recommended only by a certain appeal to imagination." This method has gradually crept into philosophy through a critical scrutiny of mathematics. It is imbued with the essential spirit of a theoretical science based on experimental results.

The fact that the scientific method is encroaching upon the domain of philosophy will raise the question as to how far it is able to go towards solving the problems of metaphysics. It appears already to have been quite successful in dealing with the notions of continuity and infinity. But that it shall undertake to solve all the metaphysical problems is unlikely. What is more probable is that it shall pronounce many of them meaningless or else out of reach of exact investigation and consequently leave them to one side.

Returning now to the more special problems of science proper, let us inquire what is the present outlook for definite achievement in research. There are various types of answers to this question and various types of persons who make them. Some take an enthusiastically optimistic view of the situation. Others are pessimistic, though there seems to be less ground for pessimism now than there was fifteen or twenty years ago. Some of these pessimists believe that research is about to run out, at least in their own fields. They see nothing vital remaining to be done or else they feel helpless in the presence of a problem which is conceived. The persons who have this pessimistic feeling may be divided into two classes.

In the first place, there are those who have not attempted research and therefore have no first-hand acquaintance with its methods and problems and difficulties. At most they can see as through a glass darkly. One feels that their pessimism will prevent them from ever seeing as face to face. Some of these persons are so pronounced in their views as to believe that research has never made any really significant progress. They reach this opinion from quasi-philosophical considerations and not from an examination of the facts. It is unnecessary to refute these persons. Their judgment of matters of research properly has no weight at all among men who are actually engaged in extending the bounds of knowledge.

In the second place, our group of pessimists include those who have themselves undertaken research and have been unsuccessful in their venture. There is an obvious reason for their opinion; but it is one which makes no contribution toward answering the question as to the general outlook for definite achievement in research.

Over against these pessimists there is a large and ever-increasing body of enthusiastic researchers. They believe in to-morrow because they saw good things yesterday and have seen better ones to-day. It is hard for them to perceive how any one can fail to feel the expansion of growth in the midst of which he is living. To them it is the most natural of all expectations to think that we are just now on the eve of great developments. What is the ground of their confidence, insofar as it is not temperamental?

It is not that they have a vision of easy conquest. It can not be doubted for a moment that difficulties of the most serious sort confront us in scientific investigations. No one of these optimists can see the goal which he confidently expects science to attain. But there are some things which he can see, namely, past achievements and the circum-

stances under which the work has heretofore made progress.

It is the examination of these things which gives rise to such optimism, and especially of those of them which belong to the last few years. We shall not have time to take up these matters in detail so as to examine the events one by one; we can only indicate their general characteristics, leaving it to the reader to supply the concrete individual instances.

Let us ask: What is the leading characteristic, in the infancy of their development, of those processes and results of thought which have most profoundly influenced human progress?

To attempt a full discussion of this problem would carry us too far aside. But a partial answer lies close at hand. Great steps forward have usually been taken in a way which was not expected and in a field of mental activity where the processes and results of thought had assumed an apparently fixed form. In such a region there had been for a time a seething of thought with frequent eruption of new theory; but at last everything had come to a state of quiescence. Apparently, nothing more was to be expected from that quarter. But the appearance was false; a fresh development came with astonishing swiftness.

Often at a moment when least expected new and vital discoveries are made. Thought is ruthlessly jostled out of the rut into which it has fallen. A state of uncertainty and uneasiness ensues. Restlessly the mind seeks new verities to which to fix itself. There is a general shaking up of its content of thought. The old bottles are not strong enough for the new wine of new truth and are burst asunder. This quickening of thought, this expansion into larger conception, this is the leading characteristic of fundamental advances in human thinking.

This which I have just described is to my mind precisely the leading characteristic of several important theories of modern science. There has been a ruthless shaking up of the whole substructure; uncertainty, and even uneasiness, have arisen in many quarters; in some fields there is no longer any one who believes that he knows what should be expected. An eminent scientist who, a few years ago, was authority for the statement that the future advancement of physics was to be looked for in the fifth decimal place is now advising younger men to try all sorts of "fool experiments." This is an indication of the spirit of the times. We find indeed that our power over nature is increasing and that we can make better predictions than ever before; but we no longer have the faith which we once had in our theoretical explanations.

In recent years one surprise after another has come with such rapidity that we no longer know what it is to be orthodox in scientific theory.

A new liberty—some will say, a new license—in theorizing has sprung up everywhere. The boldness of some of the new hypotheses is amazing, even disconcerting. If ever they come into a general acceptance they will give rise to an expansive development of the human mind in virtue of our attempt to understand the philosophic significance of the new movements. They will require revision of our ways of thinking, and will thus mark a new stage in human progress.

An examination of the discoveries which have given rise to this sort of thing will lead to the observation that many of them were made in such unexpected ways that one almost feels as if they came about by accident. In fact there seems to be a certain element of haphazard in all scientific discoveries. We have not yet learned how to make a systematic and all-embracing search through fields of thought either old or new. Our best discoveries are frequently made in territory over which we have trodden many times before.

What are we to conclude from the fact that our particular discoveries are so often hit upon almost by chance and that we have looked about so nearly at random and have found such things? Let us answer by raising another question. Suppose that it had been true twenty years ago that only a few fundamental facts yet remained to be discov-

ered, in physics for instance, and suppose further that men had set about, as indeed they have, to try all sorts of "fool experiments"; then, in view of the infinite multiplicity of things which they might have tried, what is the probability that they would have discovered all or nearly all of the fundamentally new facts which twenty years ago were yet to be brought to light? According to the theory of probability, this chance is practically nil. Let us put with this result the further fact that for many hundred years men had been looking at phenomena with care and had not found the important facts discovered in this twenty-year period. Then, in view of all this we can only conclude that it is extremely probable that there is yet an unlimited, or at least a very great, number of fundamental facts still to be discovered. We can hardly refuse to draw the further conclusion that all we know at present is only a mere fragment of what we shall ultimately find out.

We can indicate the immediate prospect more precisely by a consideration of the present state of physics which I believe now stands in an enviable position with respect to all science and all philosophy—in fact, with respect to every body of doctrine whose development makes for human progress. In recent years it has undergone a marvelous rejuvenation, into the detail of which we can not now enter. It requires no eye of prophecy to see that this is certain to make itself felt in valuable advances in astronomy and geology and to lead the way to new and fundamental conquests in chemistry and biology. All branches of the sciences of phenomena should sit at the feet of the new physics in order to get in touch with her most recent discoveries and to carry them over to their consequences in other special domains of research.

All indications point to magnificent conquests of research in the immediate future and for many years to come. An analysis of the past gives us a strong assurance that there are many important things yet to be discovered. The progress of the preceding decade shows that we have in hand tools that have been effective, and we can hardly suppose that they have just now finished their work when we consider the sort of achievements which have just been made. Notwithstanding that the war in Europe will cut off many young men of enthusiasm and power and hinder the work of all investigators on that continent, it is yet true that there is an enthusiastic body of workers, especially in America, still carrying on their silent conquests which will take a place alongside the great achievements of the race. It is a pleasure to know that there is such an organization as this society to foster a work of this sort. I am glad that so many of us have entered upon the undertaking already and I hope that young men and women of promise will see a possibility of labor toward the good of the whole future of mankind and will lay their lives and their energies upon the altar of service in science.

R. D. CARMICHAEL

THE PHILOSOPHY OF BIOLOGY: VITALISM VERSUS MECHANISM[1]

IN comparison with mathematicians and physicists, biologists have contributed little to philosophical literature, notwithstanding the close relations existing between their science and philosophy. The most notable instance of recent years has been Driesch, whose attempts at philosophical commentary and interpretation seem, however, to have given on the whole little satisfaction to either biologists or philosophers. Bergson—"the biological philosopher," as Driesch calls him—bases much of his doctrine on biological data, and the use of such data appears to be becoming more frequent among philosophers. Lately professed biologists have shown somewhat more tendency to enter the field of philosophical discussion; and it is remarkable that when they do so they often adopt a vitalistic point of view. Haldane's "Mechanism, Life and Personality" is one recent illustration of this tendency, and the present book of Johnstone's is another.

[1] "The Philosophy of Biology," by James Johnstone, D.Sc., Cambridge University Press, 1914.